Review answers Manuscript tc-2024-1708

Answer to the Referee #1

Lines 518-520: The previous version stated what layers were not included in the analysis. The revised version is misleading. In a natural snowpack, more recent studies than Boyne and Fisk show that ice layers are not often impermeable as lab studies have suggested. Furthermore, other layer interfaces may be more effective at retaining water resulting in the high variability of LWC (Techel and Pielmeier, 2011 is a great example). While the authors try to justify the exclusion of comparisons in the field with the Boyne and Fisk laboratory study. I disagree. All of the data for comparison must be included in the mean difference and standard deviation %s shown.

Thank you for your comment, we appreciate the opportunity to further clarify this important point. As noted by Boyne and Fisk (1987), *"Attempts to compare measurement methods in stratified snowpack have not been successful because of the spatial variability of liquid water [Denoth et al., 1984; Boyne and Georte, 1987]."*. To mitigate this, and following the general procedure used by Boyne and Fisk (1987), we excluded sampling points where conditions of LWC were likely inconsistent (as is happening for the two excluded points). While our paper already discussed the differences between Dentohmeter and calorimetric measurements (comparing also with previous studies e.g., Perla 1991), the primary goal of this exercise was to indirectly compare our findings with those of Boyne and Fisk (1987), so we need to be sure to undergo to the same hypothesis. To enhance clarity, we have revised the sentence as follows:

"Following the procedure outlined in Boyne and Fisk (1987), we analyzed 16 measurements where both instruments sampled the same LWC conditions for the profile shown in Fig. 9, excluding the measurements at H=87 and H=0 cm. These points were omitted due to the high horizontal variability in LWC, which made it impossible to ensure that we were sampling identical conditions (see *Techel and Pielmeier, 2011*). The results showed a mean difference of 0.96% and a standard deviation of 1% between the two methods, aligning closely with previous findings for alcohol calorimeters and Denothmeters."

Lines 546-547: Please remove this statement. I think the preference of one method over another is not justified with only two pits of similar conditions. Much more data and a third method for comparison would be necessary to make this claim.

Even though we think that our previous statement was already expressed in a neutral form we further soften our statement as: "Although the coherence of the profile along the vertical axis and its correlation with density and stratigraphy are in favor of calorimetric measurements, additional data and analysis are required to fully support the preference of the calorimetry profile over the Denothmeter."

Other minor concerns to be addressed:

Line 414: How did you dry the calorimeter and what insights do you have? The authors gave a great response in the reviewer response document, but nothing was added to the manuscript which was the intent of the question.

Considering our previous answer, we modified the text in this way: "13. Empty and dry the calorimeter thoroughly before subsequent measurements. While carefully drying the calorimeter is advised, the residual water inside the calorimeter will be included in the mass and temperature measurement of the hot water, whereas the potential water or snow attached on the outside part of the calorimeter has to be clean out (even though their mass is in general negligible)."

Lines 607-609: This is a misleading statement as Webb et al. (2021) did compensate for energy losses to the calorimeter, albeit not using a calorimeter constant, the compensation was still applied. The present manuscript is certainly a better way of doing it than Webb et al. (2021), but they did address it.

Thank you for the feedback. However, our statement was specifically addressing the use of a calorimetric constant to compensate for the factors influencing the melting process inside the calorimeter, rather than the compensation for thermal dispersions from the calorimeter with the external environment. While Webb et al. (2021) discussed in the section “Uncertainty and Future Work” about the potential energy losses, they do not appear to incorporate a compensation for the energy exchange with the internal wall of the calorimeter in their formulation.